The storms generated in the Bay of Bengal afford unusual facilities for studying the genesis of cyclones. We have observatories round three sides of the bay, and the sea is, at all times of the year, traversed in all directions by numerous steamers and sailing vessels, which have furnished abundant logs. Did parallel currents play any important part in the production of the vortices, they could not possibly escape our notice. But we find that the antecedent conditions of a cyclone are light, variable winds and calms, with a nearly uniform barometric pressure all round the coasts; and only to the south, in the neighbourhood of the equator, is there any considerable movement of the air, viz., from the west. Under these circumstances, the pressure falls over some part of the bay; most frequently in the middle, and especially to the west of the Andamans. This region of falling pressure is characterised by torrential rains, with, at first, but little wind; but after a day or two (sometimes several days) of this weather, a cyclonic circulation is set up, with a marked indraught in the neighbourhood of the cyclone cradle, and thus the storm is generated.

Having regard to these facts of observation, it appears to me that it is in the condensation of the heavy rain (constantly noted as "torrential" in ships' logs) over the cyclone cradle, that we have the real source of the energy of the incipient storm. The hypothesis of parallel currents fails to provide this energy; for it is obvious that the deviation of the winds under the influence of the earth's rotation can furnish no energy, and can produce only a moderate barometric depression, the amount of which depends on the velocity of the original winds, and can be calculated by Ferrel's law. When this is reached, the system of pressures and wind-movements will be in equilibrium. If (and this I am not prepared to deny) a cyclone is sometimes generated between parallel currents, it must be that the energy of the storm is supplied from some other source, and what this is, is, I think, clearly indicated by the case of the Bay of Bengal storms.

It was first noticed by Mr. Eliot as a general fact, that, during the formation of a cyclone in the Bay of Bengal, little or no rain falls on the coasts; while, as already remarked, it is exceedingly heavy over the place of the storm's origin. The vapour generated over the bay, which, under other circumstances would be carried away by the winds and condensed over the land, is then condensed over the bay itself. The quantity of latent heat thus set free is enormous; and as Reye has shown, is ample for the production of the most violent cyclone. It would be erroneous to say that the air is thereby warmed and expanded, because, of course, the very fact of its vapour being condensed proves that it must be cooling; but Welsh's and Glaisher's balloon observations show that in a cloud-laden atmosphere, the vertical decrement of temperature is slow, as compared with that in a clear atmosphere; and the same fact is further illustrated by the temperature of hill-stations in the wetter parts of the Himalaya as compared with that of the plains at their foot. At Darjiling, for instance, the temperature from June to August (the season of greatest cloudiness and heaviest rainfall) is only 17° or 18° below that of Goalpara. In February and March (the driest months) it is between 23° and 24°. The explanation of these facts is afforded by the different rates of cooling experienced by saturated and unsaturated air, respectively, in an ascending mass of air which is expanding under a constantly diminishing pressure. Saturated, *i.e.*, cloud-laden and rain-condensing air at 80° cools only 20° by the work done during its ascent from the plane of 30 inches pressure to that of 20 inches pressure, say through 10,000 feet; whereas unsaturated air cools about 54° in the same ascent, the exact amount varying slightly according to the quantity of vapour it contains. The latent heat set free in the condensation of cloud and rain is then entirely used up in the work of expanding the cloud-laden air under a constantly diminishing pressure, and economises more than half (indeed, in the case adduced, nearly two-thirds) of the sensible heat which furnishes the energy to unsaturated air. Hence, an ascensional current, however small, once set in action in a nearly saturated atmosphere, such as exists over the Bay of Bengal during the formation of a cyclone, carries warm air to a greater height than in the clearer and drier atmosphere around the coasts, relatively raising the mean temperature of the former air-column, and of course reducing its weight. This differential effect goes on increasing, and the ascending current becomes more rapid, until the indraught below attains the conditions of a cyclonic storm.

Now, in the case of parallel currents, there must be between them a region of calm; and, if this is over a sea of high temperature, it is conceivable that, as in the Bay of Bengal, local condensation may proceed for a sufficient time unchecked to lead up to the formation of a cyclone; but, in that case, the cyclone will be generated, not immediately, as supposed by Mr. Barham, by the energy of the pre-existing winds, but by their affording conditions in which another and far more potent source of energy comes into play. H. F. BLANFORD

Dinard, France, July 10

The Tasimeter and Magnetisation

AFTER perusing an account, in a recent number of the *Scientific American*, of Edison's Tasimeter, it occurred to one of us to apply it to detect, and, if possible, to measure the elongation and shortening, which, as discovered by Joule, are produced in a bar of iron by magnetisation and demagnetisation. Accordingly to test whether the effect could be observed in this way, a rough specimen of the instrument was constructed, and with it some preliminary experiments made, an account of which may interest the readers of NATURE. A small cylinder, about half a centimetre in length and diameter, of the carbon used for Bunsen's cells, rested with its ends which were slightly rounded, in contact with two brass plates, one of which was fixed to a rigid upright attached to one end of the base of the instrument, while the other, resting with one end on the base, formed a spring, which in its normal position just touched the end of the carbon. A coil containing four layers of insulated wire, six turns to the layer, was wound round a tube ten centimetres long and eight millimetres in diameter, and fixed with its axis in line with that of the carbon cylinder. A piece of iron wire was then placed in the axis of the tube with one end resting against the spring, and the other in contact with the end of a screw working in a nut fixed to a rigid upright at the end of the base remote from the carbon. By means of this screw the pressure of the iron bar on the spring, and consequently of the spring on the carbon, could be varied at pleasure.

A terminal of copper wire, was attached to each of the brass plates bearing on the carbon, and joined up so that the carbon, plates, and terminals formed one of the resistances of a Wheatstone's bridge, in connection with which a battery of one Daniell's cell and a very delicate Thomson's reflecting galvanometer were used. When the iron wire forming the core of the electro-magnet had been so adjusted that there was only a very slight pressure on the carbon, the resistances of the bridge were arranged to make the deflection of the galvanometer produced by the current from the battery nearly zero. The galvanometer and battery keys were then put down and the current allowed to flow through the bridge during the remainder of the experiment. The electro-magnet was then excited by the current from three of Thomson's Tray Daniells. This produced a deflection of the image on the galvanometer scale of about fifty divisions in the direction indicating a diminution of the carbon resistance, which must have been caused by change of contact produced by increased pressure on the spring. The length of the iron core of the electro-magnet had therefore been increased by magnetisation. When the magnetising force was removed the image immediately returned to its former position. As a verification that the diminution of resistance indicated by the bridge arrangement was caused by elongation of the iron core, the adjusting screw was turned forward through a very small distance, when the deflection was found to be in the same direction as before. When the screw was brought back the image on the scale returned towards its zero. Experiments with various strengths of current gave perfectly accordant results.

We hope by replacing the comparatively rough adjusting screw by a micrometer screw to be able to make some measurements of the exact amounts of elongation or shortening produced in a piece of soft iron or steel by given changes of magnetic intensity. It may be remarked that this method of measurement could be advantageously applied in cases where the amount of change of dimensions to be discovered or measured is very small, but the force which it could be arranged to produce abundant.

University of Glasgow, July 12 ANDREW GRAY
THOMAS GRAY

Physical Science for Artists

THE curious phenomenon described by Prof. Brücke and Mr. Norman Lockyer, under the name of "les rayons de crépuscule," though rare and uncommon in the island of Ceylon, is

well-known to the natives under the title "Buddha's rays." It has also, I believe, been noticed in Cashmere. The phenomenon, which is very striking indeed under favourable conditions, is confined to the mountain region in the central parts of the above-named island, and is never, as far as I am aware, seen in the low country. It was therefore with considerable interest that I learnt that it is well known to the French, and had been seen by Mr. Lockyer at sea. In May, 1876, in a paper on "Remarkable Atmospheric Phenomena in Ceylon," read before the Physical Society and published in the *Proceedings*, I offered an explanation of this phenomenon, in accordance with the conditions under which it appeared. As this explanation is very brief, may I ask your permission to reproduce it *verbatim?*

"Not unfrequently in the mountain districts broad beams apparently of bluish light may be seen extending from the zenith downwards, converging and narrowing as they approach the horizon. This ray-like appearance is very similar to that seen before sunrise; only the point from which the rays proceed is *opposite* the sun: the rays themselves are very broad and blue in colour; and the spaces between them have the ordinary illumination of the rest of the sky. If we suppose in this instance that the lower strata of air are colder than the upper (a condition of the atmosphere which not unfrequently occurs in a tropical mountain district like that of Ceylon, where large currents of heated air sweeping up a valley cross another valley nearly at right angles and at a considerable elevation above it), the refraction spoken of in the case of Adam's Peak will be downwards instead of upwards. If, too, the observer be *below*, the veil of darkness will appear to him like a very elongated triangle, apex downwards, or broad ray, through which the blue sky beyond may be seen free from the palish illumination of the atmosphere, whilst on either side the ordinary illuminated sky will be seen. If now we suppose several isolated masses of cloud to partially obscure the sun, as was the case when I witnessed the phenomenon, we may have several corresponding inverted veils of darkness, like blue rays in the sky, all apparently converging towards the same point below the horizon. This apparent convergence of the beams is merely an effect of distance, as in the case of parallel rays of light from the rising or setting sun, the blue rays being practically parallel bands in the atmosphere devoid of illumination. It will be evident that conical-shaped clouds are not necessary to produce this effect. Isolated clouds of any massive form would be sufficient to throw the bands of shadow through the illuminated atmosphere, and refraction and perspective would do the rest. The above phenomenon is called by the Singhalese 'Buddha's rays;' and though, according to Sir Emerson Tennent, it is very varied in character and appears in different parts of the sky, yet I have only seen it when the sun was low at evening and when the rays converged to a point, apparently directly opposite the sun; and I do not think it possible for the phenomenon to be seen in any other position."

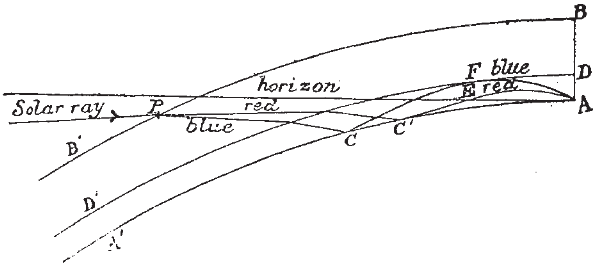
It will be seen that I thought it necessary to suppose a peculiar state of the atmosphere, in order that the shadows themselves might be sharply refracted downwards; indeed, I think, if the phenomenon can be produced under ordinary conditions, it would be much less rare in the island than it is. It may be mentioned that the sharpness and definition of the rays was most decided—far more so than the sunbeams and shadows are at sunrise or sunset—and that the position from which I observed the phenomenon was a ridge 4,500 feet high, overlooking a deep valley lying north and south, into which another valley running east and west opened at a high elevation, so that the warm air coming up the latter from the country below would overlie the cooler air of the first valley. I am not certain whether mountains a few miles to the west had any part in the production of the phenomenon, but the sun was certainly setting behind dense but isolated masses of cloud. With the above exception, it appears that my explanation is identical with that of Mr. Norman Lockyer and Prof. Brücke.

The reference in the above quotation to Adam's Peak is to the shadow which this isolated and cone-shaped mountain throws up into the sky at sunrise. The summit of this peak is 7,200 feet high, and overlooks the low country for fifty miles or more to the west. The shadow thrown by it at the moment of sunrise appears to lie horizontally over the land and sea for a distance of seventy or eighty miles. As the sun rises, the shadow rises and takes the form of a gigantic pyramid rising in the air and approaching the mountain, until at last it appears to fall over on the summit. The explanation which I offered of this was in principle identical with that of the mirage, the rays grazing the summit and sides of the mountain being refracted upwards, and thus leaving a portion of the atmosphere, similar in shape to the upper part of the Peak, unilluminated. This refraction is extremely likely to occur, as the difference in temperature between the air in the low country and on the top of the Peak is at least from 30° to 50° F. during the night and morning. The sudden vanishing of the shadow is, no doubt, due to the rays reaching the critical angle when the sun has risen to a certain height, and total internal reflection ceasing to take place. It may be mentioned that in both cases the shadows appear to be blue, but this, I imagine, is only the effect of contrast. This theory will, I believe, also explain the phenomenon of the spectre of the Brocken, which appears, I understand, under similar but less pronounced conditions.

The above appear to be very good instances of the illumination which the particles of air are capable of receiving, for it is evident that neither phenomenon would be visible if the atmosphere were perfectly pure.

Another phenomenon I shall be glad to refer to, as bearing on the subject of a perfectly clear sky at sunset. During the dry season on the West Coast of Ceylon, when the air is so free from moisture that even in houses close by the sea-shore the backs of books and sheets of paper curl up as if they had been placed before a fire, I have noticed, in looking westward over the sea immediately after sunset, that the sky has presented the appearance of an almost perfect spectrum extending from the horizon upwards for a very considerable distance towards the zenith (as far as I remember to within 20° of the zenith), a fiery red being lowest, succeeded by orange, yellow, green, and blue. With the exception of the red the colours were of the most transparent character possible; such as it seemed to me at the time it would be quite impossible to reproduce except with the prism or or diffraction lines.

The explanation which I would offer of this seems to bea directly on the question of the colour of the sky, and as it has not



I believe, been suggested, I shall be glad if you will allow it to appear. It may, however, be an old explanation, but I am not aware that it is. When a ray of sunlight passes obliquely through a thick plate of glass the issuing ray is found to have been dispersed during its passage, although on account of the overlapping of the spectra produced by the pencil it appears to the eye as white light, one edge where there is no overlapping being slightly red, and the other slightly blue. Now the atmosphere of the earth is such a parallel plate as this, and the solar rays falling on it very obliquely are dispersed into their component colours, the air being the dispersive medium. These colours cannot be observed (1) unless the obliquity is very great, for the colours overlap and white light is the result; (2) unless there is very little moisture in the air to absorb the more delicate colours and so prevent the dispersion effect being distinguished. In the case I refer to both these conditions were present in a remarkable degree. A reference to the figure will show at once the explanation offered for the spectral appearance of the sky. Let A A′ be the surface of the earth, B B′ the upper limit of the atmosphere, D D′ the upper limit of the more refracting layers of the atmosphere, A the point of observation, A X the horizon.

A solar ray striking the atmosphere at P will be refracted and dispersed, the blue ray striking the earth at C, and the red at C′. When these rays reach the surface of a sea that is nearly calm they will be reflected in various directions according to the angle at which they strike the wave-surfaces. Some will be reflected nearly vertically and will be lost in the clear sky, others will be reflected less vertically and will be subjected to the refracting influence of the lower strata of the air, the blue being refracted most and the red least. Thus the figure C F A may

represent the blue ray that suffers most refraction of all the blue rays, and C E A the red ray which suffers most refraction of all the red rays.

It thus appears that of blue and red rays reflected from the sea at the same angle, the former may reach the eye of the observer and the latter not, because, though the refraction is sufficient for the blue it is not so for the red ray, and it will be lost in the upper air. Consequently the blue rays will appear highest, and the red lowest, the other colours occupying intermediate positions according to their refrangibility. It is evident that any of these rays may be reflected too vertically from the sea, and so not be refracted to the earth again, but a considerable proportion will be thus refracted, and as has been said, more vertically inclined rays of the blue than of any other colour.

When we consider the effect of rays falling to the left of C, the phenomenon becomes more complicated. The same refraction, dispersion, and reflection take place, but the rays after reflection will mostly fall short of A, and strike the sea at various angles, producing a great variety of colour. It is not necessary for the effect that both the blue and the red from the same pencil of light should proceed to A, although this is shown for the sake of simplicity in the figure. It is sufficient if we know that blue rays, on account of their greater refrangibility, must of necessity be the highest, and the red, on account of their least refrangibility, the lowest.

If the above suggestion as to the dispersive power of the atmosphere be admitted, it is probable that the question of the colour and scintillation of stars will be directly affected by it.

Little Bromley, Manningtree, July 12 R. ABBAY

Zoological Geography—Didus and Didunculus

MR. SEARLES V. WOOD will, I trust, pardon me if I again take exception to the terms in which (*suprà*, p. 301) he still writes of *Didus* and *Didunculus*. These two birds do not belong to the same group of *Columbæ*. The fact that certain authors may have included them under the designation of "ground-doves" is no proof whatever of their relationship, any more than it is of the relationship of either to any other birds so called—for instance those of the Neotropical genus *Chamæpelia*. I have studied pretty carefully the osteology of many forms of *Columbæ* with especial reference to their affinities. *Pezophaps* and *Didus* are of course nearly allied, though even these are not congeners. *Didunculus* is at least as distinct from them as from all other *Columbæ* with the possible exception of *Otidiphaps*, which last I have not had an opportunity of examining. Furthermore, I may remark that if Mr. Wood will but look at what has been published of the habits of *Didunculus* he will find that it is as much an arboreal as a terrestrial bird, so that the name of "ground-dove" is as unhappily applied to it as is that of *Didunculus* or its ridiculous translation, "Dodlet."

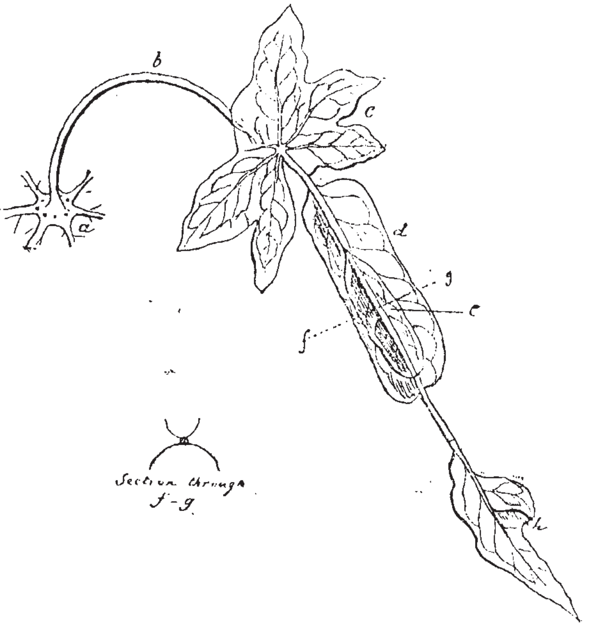
July 22 ALFRED NEWTON

Autophyllogeny

THE following case of *Autophyllogeny*, observed in a leaf of *Papaya vulgaris* (the well-known papaw-tree) appears to me of sufficient interest to be recorded in the columns of your highly interesting journal.

The letter *a* designates the central part of the primary leaf, corresponding to the apex of the petiole on the upper side of the blade. It shows some small warty protuberances, and from amidst them rises a new petiole (*b*), about six centimetres long and one and a half millimetre thick. This new petiole bears an accessory leaf of somewhat pentagonal outline (*c*), slightly crumpled and partially concave towards the upper side (the one directed downwards in the figure), as if there had been some tendency of forming a leaf pitcher. A little onwards two boat-shaped appendices are observed (*d* and *e*), the midrib or petiole forming their keel. They are real leaf pitchers, though of a rather uncommon form. The small lateral diagram represents the shape of the transversal section through *f* and *g*. The two leaves are opposed to each other by their upper sides, which are of a dark green colour; the concave parts are their under sides, as is proved by their pale green colour, which is generally the case in the leaves of the papaw-tree. The end of the petiole bears a pointed leaf (*h*), slightly contracted, and with a pitcher-like contortion on one side. The figure is about three-fourths natural size.

The case belongs to those mentioned by Masters ("Vegetable Teratology," 355, 445) under the heads of Pleiophylly and Enation from foliar organs. His explanation is certainly correct, as there cannot be any doubt that the accessory petiole *b*, but for its development in another plane, is a true homologon of the ribs of the primary leaf, and the minute warts round its base may be regarded as small or checked beginnings in this same direction.



The described anomaly does not appear to be rare in *Papaya vulgaris*. I have observed several less-developed instances; the specimen here described was given to me by one of our students, Señor Ramon Documet. A. ERNST

Caracas, June 16

Microscopy—The Immersion Paraboloid

As I am responsible for exhibiting at the Conversazione of the Royal Society, May 1, the immersion paraboloid as being "designed by Dr. Edmunds," I should wish it to be known that, since that date, my attention has been directed to evidence establishing Mr. Wenham's priority to the invention.

Before exhibiting the paraboloid at the Royal Society, I had Dr. Edmunds' assurance that he felt justified in requesting me to describe it as designed by himself. JOHN MAYALL, Jun.

224, Regent Street, London, July 16

THE GENESIS OF LIMBS[1]

III.

I HAVE found much resemblance between the skeleton of the ventral and the dorsal fins in *Notidanus*, in *Chiloscyllium*, and in *Raia*; also between the anal and ventral fins in *Notidanus*. The ventral fins of elasmobranchs generally are so different from their pectoral fins, and so much more like the azygos fins than the pectorals are, that they serve well to bridge over the differences between the orders of fins. At the same time the value of the link is enhanced by the fact that in the very peculiar genera *Callorhynchus* and *Chimæra* the ventrals resemble the pectorals in a very remarkable and exceptional manner. But perhaps the most instructive ventral fin is that of *Polyodon*, the skeleton of which consists simply of a double series of simple parallel rays without any attachment to a pelvic cartilage which is altogether absent.

These conditions, then, appear to obliterate the distinctions which are at first apparent between the skeletons

[1] Continued from p. 311.